SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

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FRIDAY, JUNE 26, 1903.

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*MODERN VIEWS ON MATTER: THE REALIZATION OF A DREAM.**

FOR nearly a century men who devote themselves to science have been dreaming of atoms, molecules, ultramundane particles, and speculating as to the origin of matter; and now to-day they have got so far as to admit the possibility of resolving the chemical elements into simpler forms of matter, or even of refining them altogether away into ethereal vibrations or electrical energy.

This dream has been essentially a British dream, and we have become speculative and imaginative to an audacious extent, almost belying our character of a purely practical nation. The notion of impenetrable mysteries has been dismissed. A mystery is a thing to be solved—'and man alone can master the impossible.' There has been a vivid new start. Our physicists have remodeled their views as to the constitution of matter and as to the complexity if not the actual decomposability of the chemical elements. To show how far we have been propelled on the strange new road, how dazzling are the wonders that waylay the researcher, we have but to recall—matter in a fourth state, the genesis of the elements, the dissociation of

* An address delivered before the Congress of Applied Chemistry at Berlin, June 5, 1903, from proof under correction.

the chemical elements, the existence of bodies smaller than atoms, the atomic nature of electricity, the perception of electrons, not to mention other dawning marvels far removed from the lines of thought usually associated with English chemistry.

The earliest definite suggestion in the last century of the possible compound nature of the metals occurs in a lecture delivered in 1809* by Sir Humphry Davy at the Royal Institution. In that memorable lecture he speculated on the existence of some substance common to all the elements, and he averred that "If such generalizations should be supported by facts, a new, a simple and a grand philosophy would be the result. From the combination of different quantities of two or three species of ponderable matter we might conceive all the diversity of material substances to owe their constitution."

Again, in 1811, he said:† "It will be useless to speculate upon the consequences of such an advancement in chemistry as that of the decomposition and composition of the metals. * * * It is the duty of a chemist to be bold in pursuit. He must not consider things as impracticable merely because they have not yet been effected. He must not regard them as unreasonable because they do not coincide with popular opinion. He must recollect how contrary knowledge sometimes is to what appears to be experience. * * * To inquire whether the metals be capable of being decomposed and composed is a grand object of true philosophy."

Davy first used the term 'radiant matter' about 1809, but chiefly in connection with what is now called radiation. He also used the term in another sense, and the following passage‡ in its clear forecast is prophetic of the modern electron: 'If particles of gases were made to move in free space with an almost infinitely great velocity—*i. e.*, to become radiant matter—they might produce the different species of rays, so distinguished by their peculiar effects.'

In his lectures at the Royal Institution, in 1816, 'On the General Properties of Matter,' another prescient chemist, Faraday, used similar terms when he said: "If we conceive a change as far beyond vaporization as that is above fluidity, and then take into account also the proportional increased extent of alteration as the changes rise, we shall, perhaps, if we can form any conception at all, not fall far short of radiant matter; and as in the last conversion many qualities were lost, so here also many more would disappear." again, in one of his early lectures he strikes a forward note: "At present we begin to feel impatient, and to wish for a new state of chemical elements. To decompose the metals, to reform them, and to realize the once absurd notion of transmutation, are the problems now given to the chemist for solution."

But Faraday was always remarkable for the boldness and originality with which he regarded generally accepted theories. In 1844 he said, "The view that physical chemistry necessarily takes of atoms is now very large and complicated; first many elementary atoms—next compound and complicated atoms. System within system, like the starry heavens, *may be right* —but may *be all wrong.*"

A year later Faraday startled the world by a discovery to which he gave the title 'On the Magnetization of Light and the Illumination of the Magnetic Lines of Force.' For fifty years this title was misunderstood and was attributed to enthusiasm or confused ideas. But to-day

* 'Works of Sir Humphry Davy,' Vol. VIII., p. 325.

† *Loc. cit.*, Vol. VIII., p. 330.

‡ *Loc. cit.*, Vol. VIII., p. 349.

we begin to see the full significance of the Faraday dream.

In 1879, in a lecture I delivered before the British Association* at Sheffield, it fell to my lot to revive 'Radiant Matter.' I advanced the theory that in the phenomena of the vacuum tube at high exhaustions the particles constituting the cathode stream are not solid, nor liquid, nor gaseous, do not consist of atoms propelled through the tube and causing luminous, mechanic or electric phenomena where they strike, 'but that they consist of something much smaller than the atom—fragments of matter, ultra-atomic corpuscles, minute things, very much smaller, very much lighter than atoms—things which appear to be the foundation stones of which atoms are composed.'†

I further demonstrated that the physical properties of radiant matter are common to all matter at this low density—'Whether the gas originally under experiment be hydrogen, carbon dioxide or atmospheric air, the phenomena of phosphorescence, shadows, magnetic deflection, etc., are identical.' Here are my words, written nearly a quarter of a century ago: "We have actually touched the borderland where matter and force seem to merge into one another‡—the shadowy realm between the known and unknown. I venture to think that the greatest scientific problems of the future will find their solution in this borderland, and even beyond; here, it seems to me, lie ultimate realities, subtle, far-reaching, wonderful."

It was not till 1881 that J. J. Thomson established the basis of the electrodynamic theory. In a very remarkable memoir in the *Philosophical Magazine* he explained the phosphorescence of glass under the influence of the cathode stream by the nearly abrupt changes in the magnetic field arising from the sudden stoppage of the cathode particles.

The now generally accepted view that our chemical elements have been formed from one primordial substance was advocated in 1888 by me when president of the Chemical Society,* in connection with a theory of the genesis of the elements. I spoke of 'an infinite number of immeasurably small ultimate—or, rather, ultimatissimate—particles gradually accreting out of the formless mist, and moving with inconceivable velocity in all directions.'

Pondering on some of the properties of the rare elements, I strove to show that the elementary atoms themselves might not be the same now as when first generated—that the primary motions which constitute the existence of the atom might slowly be changing, and even the secondary motions which produce all the effects we can observe—heat, chemic, electric and so forth—might in a slight degree be affected; and I showed the probability that the atoms of the chemical elements were not eternal in existence, but shared with the rest of creation the attributes of decay and death.

The same idea was expanded at a lecture I delivered at the Royal Institution in 1887, when it was suggested that the atomic weights were not invariable quantities.

I might quote Mr. Herbert Spencer, Sir Benjamin Brodie, Professor Graham, Sir George Stokes, Sir William Thomson (now Lord Kelvin), Sir Norman Lockyer, Dr. Gladstone and many other English *savans* to show that the notion—not necessarily of the decomposability but at any rate of the complexity of our supposed elements

* 'British Association Reports,' Sheffield Meeting, 1879. *Chemical News*, Vol. XL., p. 91. *Phil. Trans. Roy. Soc.*, 1879, Part I., p. 585. *Proc. Roy. Soc.*, 1880, No. 205, p. 469.

† Sir O. Lodge, *Nature*, Vol. LXVII., p. 451.

‡ 'Matter is but a mode of motion' (*Proc Roy. Soc.*, No. 205, p. 472).

* Pres. Address to Chem. Soc., March 28, 1888.

has long been 'in the air' of science, waiting to take more definite development. Our minds are gradually getting accustomed to the idea of the genesis of the elements, and many of us are straining for the first glimpse of the resolution of the chemical atom. We are eager to enter the portal of the mysterious region too readily ticketed 'Unknown and Unknowable.'

Another phase of the dream now demands attention. I come to the earlier glimpses of the electric theory of matter.

Passing over the vague speculations of Faraday and the more positive speculations of Sir William Thomson (now Lord Kelvin), one of the earliest definite statements of this theory is given in an article in the *Fortnightly Review* for June, 1875, by W. K. Clifford—a man who in common with other pioneers shared that 'noblest misfortune of being born before his time.' 'There is great reason to believe,' said Clifford, 'that every material atom carries upon it a small electric current, *if it does not wholly consist of this current.*'

In 1886 when president of the Chemical Section of the British Association, in a speculation on the origin of matter, I drew a picture of the gradual formation of the chemical elements by the workings of three forms of energy—electricity, chemism and temperature—on the 'formless mist' (protyle*), wherein all matter was in the preatomic state—potential rather than actual. In this scheme the chemical elements owe their stability to being the outcome of a struggle for existence—a Darwinian development by chemical evolution—a survival of the most stable. Those of lowest atomic weight would first be formed, then those of intermediate weight, and finally the elements having the highest atomic weights, such as thorium and uranium. I spoke of the 'dissociation point' of the elements. "What comes after uranium?" I asked. And I answered back, "The result of the next step will be * * * the formation of * * * compounds the dissociation of which is not beyond the powers of our terrestrial sources of heat." A A dream less than twenty years ago, but a dream which daily draws nearer to entire and vivid fulfilment. I will presently show you that radium, the next after uranium, does actually and spontaneously dissociate.

The idea of units or atoms of electricity —an idea hitherto floating intangibly like helium in the sun—can now be brought to earth and submitted to the test of experiment.* Faraday, W. Weber, Laurentz,

* We require a word, analogous to protoplasm, to express the idea of the original primal matter existing before the evolution of the chemical elements. The word I venture to use is composed of *πρό* (*earlier than*) and *ὕλη* (*the stuff of which things are made*).

* "The equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity; * * * it being the electricity which *determines* the equivalent number, *because* it determines the combining force. Or, if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action, have equal quantities of electricity naturally associated with them." Faraday's 'Experimental Researches in Electricity' par. 869, January, 1834.

"This definite quantity of electricity we shall call the molecular charge. If it were known it would be the most natural unit of electricity." Clerk Maxwell's 'Treatise on Electricity and Magnetism,' first edition, Vol. I., 1873, p. 311.

"Nature presents us with a single definite quantity of electricity. * * * For each chemical bond which is ruptured within an electrolyte a certain quantity of electricity traverses the electrolyte, which is the same in all cases." G. Johnstone Stoney, 'On the Physical Units of Nature,' British Association Meeting, Section A, 1874.

"The same definite quantity of either positive or negative electricity moves always with each univalent ion, or with every unit of affinity of a multivalent ion." Helmholtz, Faraday Lecture, 1881.

Gauss, Zöllner, Hertz, Helmholtz Johnstone Stoney, Sir Oliver Lodge, have all contributed to develop the idea—originally due to Weber—which took concrete form when Stoney showed that Faraday's law of electrolysis involved the existence of a definite charge of electricity associated with the ions of matter. This definite charge he called an electron. It was not till some time after the name had been given that electrons were found to be capable of existing separately.

In 1891, in my inaugural address as president of the Institution of Electrical Engineers,* I showed that the stream of cathode rays near the negative pole was always negatively electrified, the other contents of the tube being positively electrified, and I explained that 'the division of the molecule into groups of electro-positive and electro-negative atoms is necessary for a consistent explanation of the genesis of the elements.' In a vacuum tube the negative pole is the entrance and the positive pole the exit for electrons. Falling on a phosphorescent body, yttria, for instance, —a collection of Hertz molecular resonators—the electrons excite vibrations of, say, 550 billion times a second, producing ether waves of the approximate length of 5.75 ten-millionths of a millimeter, and occasioning in the eye the sensation of citron-colored light. If, however, the electrons dash against a heavy metal or other body which will not phosphoresce, they produce ether waves of a far higher frequency than light, and are not continuous vibrations, but, according to Sir George Stokes, simple shocks or solitary impulses; more like discordant shouts as compared with musical notes.

During that address an experiment was shown which went far to prove the dissociation of silver into electrons and positive atoms.* A silver pole was used, and near it in front was a sheet of mica with a hole in its center. The vacuum was very high, and when the poles were connected with the coil, the silver being negative, electrons shot from it in all directions, and passing through the hole in the mica screen, formed a bright phosphorescent patch on the opposite side of the bulb. The action of the coil was continued for some hours, to volatilize a certain portion of the silver. Silver was seen to be deposited on the mica screen only in the immediate neighborhood of the pole; the far end of the bulb, which had been glowing for hours from the impact of electrons, being free from silver deposit. Here, then, are two simultaneous actions. Electrons, or radiant matter shot from the negative pole, caused the glass against which they struck to glow with phosphorescent light. Simultaneously, the heavy positive ions of silver, freed from negative electrons, and under the influence of the electrical stress, likewise flew off and were deposited in the metallic state near the pole. The ions of metal thus deposited in all cases showed positive electrification.†

In the years 1893–1895 a sudden impulse was given to electric vacuum work by the publication in German of the remarkable results obtained by Lenard and Röntgen, who showed that the phenomena inside the vacuum tube were surpassed in interest by what took place outside. It is not too much to say that from this date what had been a scientific conjecture became a sober reality.

"Every monad atom has associated with it a certain definite quantity of electricity; every dyad has twice this quantity associated with it; every triad three times as much, and so on." O. Lodge, 'On Electrolysis,' *British Association Report*, 1885.

* 'Electricity in Transitu: from Plenum to Vacuum,' *Journ. Inst Electrical Engineers*, Vol. XX., p. 10, January 15, 1891.

* In describing the experiment, one of fundamental importance, modern terms are employed.

† *Proc. Roy. Soc.*, Vol. LXIX., p. 421.

Faraday, in 1862, long and ardently sought for a visible relation between magnetism and light which in 1845 he had foreshadowed. But his instrumental means were too feeble, and it was not till 1896 that Zeeman showed a spectrum line could be acted on by a magneic field. A spectrum line is caused by motion of the electron. A magnetic field resolves this motion into other component motions, some slower, others quicker, and thus causes a single line to split into others of greater and less refrangibility than the parent line.

One important advance in theoretic knowledge has been obtained by Dewar, the successor of Faraday in the classic laboratories of the Royal Institution. Soon after Röntgen's discovery Dewar found that the relative opacity to the Röntgen rays was in proportion to the atomic weights of bodies, and he was the first to apply this principle to settling a debated point in connection with argon. Argon is relatively more opaque to the Röntgen rays than either oxygen, nitrogen or sodium, and from this Dewar inferred that the atomic weight of argon was twice its density relative to hydrogen. In the light of to-day's researches on the constitution of atoms, it is impossible to overestimate the importance of this discovery.

In 1896 Becquerel, pursuing the masterly work on phosphorescence inaugurated by his illustrious father, showed that the salts of uranium constantly emit emanations which have the power of penetrating opaque substances and of affecting a photographic plate in total darkness, and of discharging an electrometer. In some respects these emanations, known as Becquerel rays, behave like rays of light, but they also resemble Röntgen rays. Their real character has only recently been ascertained, and even now there is much that is obscure and provisional in the explanation of their constitution and action.

Following closely upon Becquerel's work came the brilliant researches of M. and Mme. Curie, on the radio-activity of bodies accompanying uranium.

Hitherto have been recounting isolated instances of scientific speculation with apparently little relation to one another. The existence of matter in an ultra-gaseous state; material particles smaller than atoms; the existence of electrical atoms or electrons; the constitution of Röntgen rays and their passage through opaque bodies; the emanations from uranium; the dissociation of the elements—all these isolated hypotheses are now focused and welded into one harmonious theory by the discovery of radium.

> "Often do the spirits
> Of great events stride on before the events,
> And in to-day already walks to-morrow."

No new discovery is ever made without its influence ramifying in all directions and explaining much that before had been mystifying. Certainly no discovery of modern times has had such wide-embracing consequences, and thrown such a flood of light on broad regions of hitherto inexplicable phenomena, as this discovery of M. and Mme. Curie and M. Bémont, who patiently and laboriously plodded along a road bristling with difficulties almost insuperable to others who, like myself, have toiled in similar labyrinths of research. The crowning point of these labors is radium.

Let me briefly recount some of the properties of radium, and show how it reduces speculations and dreams, apparently impossible of proof, to a concrete form.

Radium is a metal of the calcium, strontium and barium group. Its atomic weight according to C. Runge and J. Precht is probably about 258. In this case it occupies the third place below barium in my

lemniscate spiral scheme of the elements,* two unoccupied gaps intervening.

The spectrum of radium has several well-defined lines; these I have photographed and have also measured their wave-lengths. Two especially are strong and characteristic. One at wave-length 3,649.71 and the other at wave-length 3,814.58. These lines enable radium to be detected spectroscopically.

The emanations cause soda-glass to assume a violet color, and they produce many chemical changes. Their physiological action is strong, a few milligrams brought near the skin in a few hours producing a wound difficult to heal.

The most striking property of radium is its power to pour out torrents of emanations bearing a certain resemblance to Röntgen rays, but differing in important points.

The emanations from radium are of three kinds. One set is the same as the cathode stream, now identified with free electrons —atoms of electricity projected into space apart from gross matter—identical with 'matter in the fourth or ultragaseous state,' Kelvin's 'satellites,' Thomson's 'corpuscles' or 'particles'; Lodge's 'disembodied ionic charges, retaining individuality and identity.' These electrons are neither ether-waves nor a form of energy, but substances possessing inertia (probably electric). Liberated electrons are exceedingly penetrating. They will discharge an electroscope when the radium is ten feet or more away, and will affect a photographic plate through five or six millimeters of lead and several inches of wood or aluminium. They are not readily filtered out by cotton-wool; they do not behave as a gas, *i. e.*, they have not properties dependent on intercollisions, mean free path, etc.; they act more like a fog or mist, are mobile and carried about by a current of air to which they give temporary conducting powers, clinging to positively electrified bodies and thereby losing mobility, and diffusing on the walls of the containing vessel if left quiet.

Electrons are deviable in a magnetic field. They are shot from radium with a velocity of about one tenth that of light, but are gradually obstructed by collisions with air atoms, so that some become much slowed, and then are what I formerly called loose and erratic particles, which diffuse about in the air, and give it temporary conducting powers. These can turn corners, can be concentrated by mica cones into a bundle and then produce phosphorescence.

Another set of emanations from radium are not affected by an ordinarily powerful magnetic field, and are incapable even of passing through thin material obstructions. These emanations have about one thousand times the energy of those radiated by the deflectable particles. They render air a conductor and act strongly on a photographic plate. Their mass is enormous in comparison with that of the electrons, and their velocity is probably as great when they leave the radium, but, in consequence of their greater mass, they are less deflected by the magnet, are easily obstructed by obstacles, and are sooner brought to rest by collisions with air atoms. The Hon. R. B. Strutt* was the first to affirm that these non-deflectable rays are the positive ions moving in a stream from the radioactive body.

Rutherford has shown that these emanations are slightly affected in a very powerful magnetic field, but in an opposite direction to the negative electrons. They are therefore proved to be positively charged bodies moving with great velocity. For

* *Proc. Roy Soc.*, Vol. LXIII., p. 408.

* *Phil. Trans. R. S.*, A, 1901, Vol. CXCVI., p. 525.

the first time Rutherford has measured their speed and mass, and he shows they are ions of matter moving with a speed of the order of that of light.

There is also a third kind of emanation produced by radium. Besides the highly penetrating rays deflected by a magnet, there are very penetrating rays not at all affected by magnetism. These accompany the previous emanations, and are Röntgen rays—ether vibrations—produced as secondary phenomena by the sudden arrest of velocity of the electrons by solid matter, producing a series of Stokesian 'pulses' or explosive ether waves shot into space.

Many lines of argument and research tending towards the same point give trustworthy data by which to calculate the masses and velocities of these different particles. I must deal with big figures, but big and little are relative, and are only of importance in relation to the limitations of our senses. I will take as the standard the atom of hydrogen gas—the smallest material body hitherto recognized. The mass of an electron is 1/700th of an atom of hydrogen, or 3×10^{-26} grm., according to J. J. Thomson, and its velocity is 2×10^{9} centimeters per second, or two thirds that of light The kinetic energy per milligram is 10^{17} ergs, about three and a half million foot-tons. Becquerel has calculated that one square centimeter of radio-active surface would radiate into space one gram of matter in one billion years.

The positively electrified masses or ions are enormously great in comparison with the size of the electron. Sir Oliver Lodge illustrates it thus: If we imagine an ordinary sized church to be an atom of hydrogen, the electrons constituting it will be represented by about 700 grains of sand each the size of an ordinary full-stop (350 positive and 350 negative) dashing in all directions inside, or, according to Lord Kelvin, rotating with inconceivable velocity. Put in another way; the sun's diameter is about one and a half million kilometers, and that of the smallest planetoid about 24 kilometers. If an atom of hydrogen be magnified to the size of the sun, an electron will be about two-thirds the diameter of the planetoid.

The extreme minuteness and sparseness of the electrons in the atom account for their penetration. While the more massive ions are stopped by intercollisions in passing among atoms, so that they are almost completely arrested by the thinnest sheet of matter, electrons will pass almost unobstructed through ordinary opaque bodies.

The action of these emanations on phosphorescent screens is different. The electrons strongly affect a screen of barium platinocyanide, but only slightly one of Sidot's zinc sulphide. On the other hand, the heavy, massive, non-deflectable positive ions affect the zinc sulphide screen strongly, and the barium platinocyanide screen in a much less degree.

Both Röntgen rays and electrons act on a photographic plate and produce images of metal and other substances enclosed in wood and leather, and throw shadows of bodies on a barium platinocyanide screen. Electrons are much less penetrating than Röntgen rays, and will not, for instance, show easily the bones of the hand. A photograph of a closed case of instruments is taken by radium emanations in three days, and by Röntgen rays in three minutes. The resemblance between the two pictures is slight, and the differences great.

The power with which radium emanations are endowed of discharging electrified bodies is due to the ionization of the gas through which they pass. This can be effected in many other ways; thus,

ionization is communicated to gases faintly by the splashing of water, by flames and red-hot bodies, by ultra-violet light falling on negatively electrified metals, and strongly by the passage of Röntgen rays.

According to Sir Oliver Lodge's electronic theory of matter, a chemical atom or ion has a few extra negative electrons in addition to the ordinary neutral atom, and if these negative electrons are removed it thereby becomes positively charged. The free electron portion of the atom is small in comparison with the main bulk, in the proportion in hydrogen of about 1 to 700. The negative charge consists of superadded or unbalanced electrons—one, two, three, etc., according to the chemical valency of the body—whereas the main bulk of the atom consists of paired groups, equal positive and negative. As soon as the excess electrons are removed, the rest of the atom, or ion, acts as a massive positively charged body, hanging tightly together. In a high vacuum the induction spark tears the components of a rarefied gas apart; the positively charged ions, having great comparative density are soon slowed down by collisions, while the electrons are driven from the negative pole with an enormous velocity depending on the initial electromotive force and the pressure of gas inside the tube, but approaching, at the highest exhaustions, half that of light.

After leaving the negative pole the electrons meet with a certain resistance, in a slight degree by physical collisions, but principally by reunion with the positive ions.

Since the discovery of radium and the identification of one set of its emanations with the cathode stream or radiant matter of the vacuum tube, speculation and experiment have gone hand in hand, and the two-fluid theory of electricity is gradually replaced by the original one-fluid theory of Franklin. On the two-fluid theory, the electrons constitute free negative electricity, and the rest of the chemical atom is charged positively, although a free positive electron is not known. It seems to me simpler to use the original one-fluid theory of Franklin, and to say that the electron is the atom or unit of electricity. Fleming uses the word 'co-electrons' to express the heavy positive ion after separation from the negative electron: 'We can no more,' he says, 'have anything which can be called electricity apart from corpuscles than we can have momentum apart from moving matter.' A so-called negatively charged chemical atom is one having a surplus of electrons, the number depending on the valency, whilst a positive ion is one having a deficiency of electrons. Differences of electrical charge may thus be likened to debits and credits in one's banking account, the electrons acting as current coin of the realm. On this view only the electron exists; it is the atom of electricity, and the words positive and negative, signifying excess and defect of electrons, are only used for convenience of old-fashioned nomenclature.

The electron theory fits and luminously explains Ampère's idea that magnetism is due to a rotating current of electricity round each atom of iron; and following these definite views of the existence of free electrons, has arisen the electronic theory of matter. It is recognized that electrons have the one property which has been regarded as inseparable from matter—nay, almost impossible to separate from our conception of matter—I mean inertia. Now, in that remarkable paper of J. J. Thomson's published in 1881, he developed the idea of electric inertia (self-induction) as a reality due to a moving charge. The electron therefore appears only as apparent

mass by reason of its electrodynamic properties, and if we consider all forms of matter to be merely congeries of electrons, the inertia of matter would be explained without any material basis. On this view the electron would be the 'protyle' of 1886, whose different groupings cause the genesis of the elements.

There is one more property of the emanations of radium to bring before your notice. I have shown that the electrons produce phosphorescence of a sensitive screen of barium platinocyanide, and the positive ions of radium produce phosphorescence of a screen of zinc blende.

If a few minute grains of radium salt fall on the zinc sulphide screen the surface is immediately dotted with brilliant specks of green light. In a dark room, under a microscope with a two-third-inch objective, each luminous spot shows a dull center surrounded by a diffused luminous halo. Outside the halo the dark surface of the screen scintillates with sparks of light. No two flashes succeed on the same spot, but are scattered over the surface, coming and going instantaneously, no movement of translation being seen.

If a solid piece of a radium salt is brought near the screen, and the surface examined with a pocket lens magnifying about 20 diameters, scintillating spots are sparsely scattered over the surface. Bringing the radium nearer the screen the scintillations become more numerous and brighter, until when close together the flashes follow so quickly that the surface looks like a turbulent luminous sea. When the scintillating points are few there is no visible residual phosphorescence, and the successive sparks appear 'atoms of intensest light,' like stars on a black sky. What to the naked eye seems like a uniform 'milky way,' under the lens becomes a multitude of stellar points, flashing over the whole surface.

'Polonium' basic nitrate, actinium and radio-active platinum produce a similar effect on the screen, but the scintillations are fewer. In a vacuum the scintillations are as bright as in air, and being due to inter-atomic motion they are not affected by extremes of low temperature: in liquid hydrogen they are as brilliant as at the ordinary temperature.

A convenient way to show these scintillations is to fit the blende screen at the end of a brass tube with a speck of radium salt in front about a millimeter off, and to have a lens at the other end. I propose to call this little instrument the 'spinthariscope,' from the Greek word *σπινθαρίς*,* a scintillation.

It is difficult to estimate the number of flashes of light per second. With the radium about five centimeters off the screen the flashes are barely detectable, not more than one or two per second. As the distance of the radium diminishes, the flashes become more frequent, until at one or two centimeters, they are too numerous to count, although it is evident this is not of an order of magnitude inconceivably great.

Practically the whole of the luminosity on the blende screen, whether due to radium or 'polonium,' is occasioned by emanations which will not penetrate card. These are the emanations which cause the scintillations, and the reason why they are distinct on the blende and feeble on the platinocyanide screen, is that with the latter the sparks are seen on a luminous

* *'Ενθ' ἐκ νηὸς ὄρουσεν ἄναξ ἑκάεργος Ἀπόλλων,*
ἀστέρι εἰδόμενος, μέσῳ ἡματὶ τοῦ δ' ἀπὸ πολλαί
σπινθαρίδες πωτῶντο, σέλας δ' εἰς οὐρανὸν ἷκεν'.

(Here from the ship leaped the far-darting Apollo, like a star at midday, while from him flitted scintillations of fire, and the brilliancy reached to heaven.) Homer's 'Hymen to Apollo,' lines 440–442.

ground of general phosphorescence which renders the eye less able to see the scintillations.

It is probable that in these phenomena we actually witness the bombardment of the screen by the positive ions hurled off by radium with a velocity of the order of that of light. Each particle is rendered apparent only by the enormous extent of lateral disturbance produced by its impact on the sensitive surface, just as individual drops of rain falling on a still pool are not seen as such, but by reason of the splash they make on impact, and the ripples and waves they produce in ever-widening circles.

Indulging in a 'scientific use of the imagination,' and pushing the hypothesis of the electronic constitution of matter to what I consider its logical limit, we may be, in fact, witnessing a spontaneous dissociation of radium—and we begin to doubt the permanent stability of matter. The chemical atom may be actually suffering a katabolic transformation; but at so slow a rate that supposing a million atoms fly off every second, it would take a century for weight to diminish by one milligram.

It must never be forgotten that theories are only useful so long as they admit of the harmonious correlation of facts into a reasonable system. Directly a fact refuses to be pigeon-holed and will not be explained on theoretic grounds, the theory must go, or it must be revised to admit the new fact. The nineteenth century saw the birth of new views of atoms, electricity and ether. Our views to-day of the constitution of matter may appear satisfactory to us, but how will it be at the close of the twentieth century? Are we not incessantly learning the lesson that our researches have only a provisional value? A hundred years hence shall we acquiesce in the resolution of the material universe into a swarm of rushing electrons?

This fatal quality of atomic dissociation appears to be universal and operates whenever we brush a piece of glass with silk; it works in the sunshine and raindrops, and in the lightnings and flame; it prevails in the waterfall and the stormy sea, and although the whole range of human experience is all too short to afford a parallax whereby the date of the extinction of matter can be calculated, protyle, the 'formless mist,' once again may reign supreme, and the hour hand of eternity will have completed one revolution.

WILLIAM CROOKES.

SCIENTIFIC BOOKS.

Index Animalium, sive index nominum quæ ab A. D. MDCCLVIII, generibus et speciebus animalium imposita sunt. By C. DAVIES SHERBORN. Part I., January, 1758, to December, 1800. Cambridge (England), University Press. (New York, Macmillan Co.) 1902. 8vo. Pp. lix + 1195.

All zoologists have been aware of the stupendous undertaking upon which Mr. Sherborn has been at work for the last twelve years, except for an interval during which his health was so impaired as to necessitate a temporary interruption.

The aim of the undertaking was 'to provide zoologists with a list of all the generic and specific names which have been applied by authors to animals since January 1, 1758,' together with an exact date for each page cited, and a reference 'sufficiently exact to be intelligible alike to the specialist and to the layman.' Special groups of animals have been so treated before, but this is the first work planned to include the entire animal kingdom in its scope.

Work was begun in July, 1890; in 1892 the British Association extended its support, and two years later appointed a committee to watch and advise the undertaking. Financial support has also been extended by the Royal Society and the Zoological Society of London.

In 1897, at the suggestion of Dr. Sclater, in view of the long period which must elapse before the completion of the whole manuscript, it was decided to publish that portion relating to the zoological literature of the eighteenth century, and this material drawn from some 1,300 volumes is comprised in the book now under review. When it is considered how rare many of the printed sources are, it must be considered fortunate that less than twenty titles comprise those which are still 'Libri desiderati.' Mr. Sherborn has been indefatigable in searching out obscure dates, the dates of works issued in parts, etc., and his contributions to our knowledge of the chronology of zoological literature will be gratefully appreciated by students.

There are one or two features of the scheme to which exception will undoubtedly be taken by many zoologists, such as the denial of standing to excerpts and authors' separates, which often appear years before the volume of transactions to which they belong is offered in its entirety to the public; also, the treatment of named figures as *nomina nuda,* if issued before or without a text explaining or describing them. However, if the facts and dates are fully included in the body of the bibliography, as we suppose to be the case, individual judgment can be exercised without reference to the views of the committee or compiler.

The list of works consulted covers forty-nine pages and is a most important part of the work, and we would strongly urge that in future instalments an even fuller and more explicit description of each be included, especially with regard to its relation to binomial nomenclature. What the student working out the nomenclature of a group needs is an exact statement of the facts. There will always be differences of opinion as to the use of these facts in some cases, but the judgment finally should be that of the student, and his opportunity to utilize the facts should not in any way be restricted by the views of those engaged in preparing or supervising the compilation.

Many of the works which are essential to the determination of questions of priority belong to the transition period when the Linnean system was not generally accepted and was frequently not even understood, so that it is of the first importance to the synonymist to know whether the author of such a work accepted the Linnean nomenclature or not, and, if he did not consistently accept it, the fact should be plainly stated. To cite an instance bearing on the question, a work by Moehring, 'Geschlachten d. Vogelen,' was printed in 1758. A friend, who, at my request, has consulted the only copy known to me in America, informs me that there are in the book no genera in the Linnean sense, no specific names in the modern sense whatever, only vernacular names; and the latest Linnean citation in the book is from the sixth (non-binomial) edition of Linnæus's 'Systema Naturæ.' The 'genera' of Moehring, therefore, are, like the 'genera' of Tournefort and other pre-Linnean authors, not entitled to be cited in systematic nomenclature. Yet of this in neither bibliography nor text of Mr. Sherborn do we find any intimation that Moehring's 'genera' are not regularly binomial.

Again, in the Museum Geversianum an extremely rare book with an important bearing on molluscan nomenclature, we find the majority of the animals cited under Linnean names, but the mollusks classified by a new method invented by Meuschen, all the 'generic' names being in the plural, many of them composed of two separated words.

'Genus 51' is 'Umbilici marini formes'; 'Genus 58' is 'Disci transfixi,' and so on. The former name does not appear in Mr. Sherborn's list at all, the second appears in the modified form of 'Disci-transfixus.' *Hippopodes* Meuschen, appears in Sherborn as *Hippopus,* without explanation, and the great majority of Meuschen's names are changed into forms which do not occur printed in his book at all. In his bibliography Mr. Sherborn states that "Meuschen's trinomials are his binomials plus 'forma' = 'varietas,' and are precisely similar to the trinomials used by mammalogists in the present day." I am not a 'mammalogist,' but I do not remember

ever seeing any mammalian generic names of the present day composed of three words or in the plural number. It is of course perfectly open to any one to accept Meuschen's polynomial plurals as 'genera,' if it seems good to them; the point here made is that a perfectly satisfactory bibliography should state the exact facts and leave the reader to apply them according to his own judgment.

Appreciating the immense and self-sacrificing labor devoted to this work by Mr. Sherborn and the committee, and the very great value to all working zoologists of the result; while feeling that any criticism must seem ungracious, we nevertheless believe that it is a matter of duty to insist on the importance of greater fulness in description and exactitude in citation of works in regard to which any doubt can exist. Otherwise an uncertainty which would be deplorable must rest on the published results, of such importance to every zoologist.

WM. H. DALL.

A Manual of Bacteriology. By ROBERT MUIR, M.A., M.D., F.R.C.P. (Edinburgh), Professor of Pathology, University of Glasgow, and JAMES RITCHIE, M.A., M.D., B.Sc., Reader in Pathology, University of Oxford. American edition (with additions), revised and edited from the third English edition by NORMAN MACLEOD HARRIS, M.B. (Toronto), Associate in Bacteriology, the Johns Hopkins University at Baltimore. New York, The Macmillan Company. 1903. 170 illustrations.

Every student of bacteriology is familiar with this excellent work of Muir and Ritchie, which must be regarded as one of the most comprehensive and most useful writings upon the subject, and every American student of bacteriology will welcome Dr. Harris's edition. In the preface of the American edition, Dr. Harris assures us that an endeavor has been made to add to the value of the book by giving practical expression of the best American laboratory methods and research, and at the same time to augment the general scope of the work without eliminating the personal impress of the author. Therefore, occasional alterations and additions of greater or lesser magnitude have been made throughout the book in general, but more especially in the chapters upon 'Methods of Cultivation of Bacteria,' 'Microscopic Methods—General Bacteriological Diagnosis—Inoculation of Animals,' 'Bacteria of the Air, Soil and Water—Antiseptics,' 'Typhoid Fever—Bacilli Allied to the Typhoid Bacillus' and 'Tetanus.'

Dr. Harris has so successfully introduced the added matter that it is practically impossible to differentiate his insertions from the original text, and we are pleased to observe that the original general arrangement and treatment of subjects has not been departed from. We are impressed with the care exercised by Dr. Harris in introducing new matter and bringing the book up to date, as well as by his selection of the important contributions of American writers to be introduced. We find the chapter upon 'The Methods of Cultivation of Bacteria' containing sufficient references to the work of Mr. Fuller upon 'The Standardization of Media' and the recommendations of the laboratory committee of the American Public Health Association upon the same subject. We also note with pleasure a description of Hill's 'Hanging Block Cultures,' by which the growth of bacteria upon solid media can be observed under the microscope. Stuart's 'Cover Glass Forceps' appear in the chapter upon 'Microscopic Methods.' Pitfield's method of staining flagella is considered with care. The chapter upon 'The Relation of Bacteria to Disease' has lost none of its excellency, though this chapter has always been of such a superior quality that it would be hard to find any way to improve it. Throughout the special part of the work we notice that matters of recent controversial interest are carefully, though not dogmatically, treated. The various toxic products of bacteria are mentioned in brief, commonly with the conclusion that very little is known about them, so that the student is not led astray. Likewise the importance of antitoxins and antiserums in those diseases in which their virtue is not proved, are but briefly dwelt upon. Koch's suggestion that the bovine tubercle bacillus does not infect man is discussed and Theobald Smith's pre-

vious contribution upon the subject mentioned. The matter is dismissed with the statement, 'As at present the subject is still under investigation in this and other countries, it would not be justifiable to dogmatize, but in the meantime we see no sufficient reason to depart from the view entertained up to this time, that the tubercle bacilli infecting mammals are of one and the same species, though differences in virulences obtained, and that milk containing tubercle bacilli is a highly important source of infection to the human subject.'

The other 'acid-fast' bacilli are mentioned briefly without speculation as to the probable ancestral importance of the grass bacilli of Moeller, to the smegma bacillus, lepra bacillus, tubercle bacillus and others. The chief use of tuberculin is said to be the diagnosis of tuberculosis in cattle. Concerning the new tuberculin, it is said 'Little success has attended the use of this substance as a remedial agent.' It is said that attempts to grow the leprosy bacilli outside of the body have so far been unsuccessful. Evidently the editor does not accept the recent contributions to the subject. The bacillus of rhinoscleroma is said not to stain by Gram's method.

The chapter upon 'Typhoid Fever' is excellent and the treatment of the colon bacillus unusually good. The author points out that the mere presence of the colon bacillus in water is not necessarily indicative of sewage pollution, as this organism is so widely distributed in nature. He also shows that the presence of the *Streptococcus* and *Bacillus enteriditis sporogenes* are important adjuncts in the detection of sewage. The microorganismal differences between fresh and stale sewage are also dwelt upon. Considerable attention is devoted to bacillary dysentery, the recent work of Flexner being given sufficient prominence. In the chapter upon 'Diphtheria' Dr. Harris seems doubtful whether the bacillus of Hoffman is an attenuated form of the diphtheria bacillus or a separate species, though he says: 'The possibility of the transformation of the pseudo-diphtheria (Hoffmann's) into the true diphtheria bacillus has been the subject of much controversy, but it can not be regarded as sufficiently established that such a transformation may be effected, still less that the former organism is related to the origin and spread of diphtheria.' We heartily endorse Dr. Harris's view that it might be well, when practicable, that every ragged unhealthy-looking wound, especially when contaminated with soil, should, as a matter of routine, be examined bacteriologically. Under such treatment from time to time cases of tetanus would be detected earlier and their treatment could be undertaken with more hope of success than at the present time. We have, however, not infrequently made very careful bacteriological studies of wounds, shortly afterwards followed by tetanus, in which for unknown reasons we failed to find any bacilli, and we regret that Dr. Harris does not recommend that simultaneously with this bacteriological examination a prophylactic injection of the antitoxic serum be given. We are fully convinced that by routine use of the antitetanic serum for purposes of prophylaxis many useful lives might be saved. We regret that in the chapter on yellow fever the name of Dr. Carlos Finlay does not appear. It was Dr. Finlay who originated the mosquito theory. The United States Army Commission of 1900 and 1901 simply proved it to be correct.

The chapter upon 'Immunity' is excellent, though we do not regard the space devoted to the 'lateral chain theory' of Ehrlich as sufficient, considering its importance and wide usefulness, and we also regret that no diagrammatic representation of Ehrlich's views is given. The usefulness of the book is augmented by excellent though brief chapters upon such other microparasites as molds, yeasts, the malarial organisms and the ameba coli. At the end of the volume eighteen pages of bibliography are appended in which a great deal of very useful material is stored away in such form that we doubt whether it will ever be utilized by students. There seems to be no systematic mode of reference to the literature given, and our impression is that references in the text to footnotes at the bottom of the page or to literature given at the end of each chapter is a far more useful

method of introducing bibliography into text-books.

JOSEPH MCFARLAND.
MEDICAL AND CHIRURGICAL COLLEGE,
PHILADELPHIA, PA.

SOCIETIES AND ACADEMIES.

ACADEMY OF SCIENCE OF ST. LOUIS.

AT the meeting of June 1, 1903, Drs. B. M. Bolton and D. L. Harris exhibited sections cut after infiltration with agar-agar, and described the use of this material for embedding purposes as follows:

Tissues can be readily hardened and embedded for cutting into sections in a hot solution of agar-agar containing formalin. The proportions of the mixture which have so far yielded the best results are nine parts of a five per cent. aqueous solution of agar-agar to one part formalin. This mixture can be prepared beforehand and kept indefinitely in an air-tight vessel. The agar-agar should be boiled for several hours, and after the addition of the formalin allowed to clear by sedimentation. The bits of tissue to be embedded are placed in a wide test-tube or wide-mouth vial containing the mixture previously melted. This is kept at 65–70° C. for an hour or longer, and the tissues are ready to be blocked. After attaching to blocks they are placed in strong or absolute alcohol for an hour or two and can then be cut. It is important not to use dilute alcohol. The tissues are stuck to the blocks by means of the agar-agar itself and adhere as soon as the agar becomes cold. No previous hardening of the tissues is at all necessary; fresh tissues can be placed at once into the hot agar-agar-formalin mixture; in fact, fresh tissue is more desirable than that which has been previously hardened, though this can also be readily embedded by this method. But the main advantage of the method, aside from its convenience and simplicity, is the fact that the cells of the tissues are not at all contracted or shrunken, and the ordinary methods of hardening have this effect more or less. With sections prepared from fresh tissues by this method the cell-protoplasm fills out the membrane fully, and the granules of the protoplasm, the nuclei, and the cell contours are remarkably distinct. The whole process, hardening, embedding and cutting, does not occupy more than three or four hours, where the tissues are not larger than about one centimeter in diameter.

Professor A. W. Greely presented the results of an investigation of the relations of *Paramœcia* and other protozoa to chemical and electrical stimuli. A detailed account of this investigation has been contributed to SCIENCE.

WM. TRELEASE,
Recording Secretary.

THE GEOLOGICAL SOCIETY OF WASHINGTON.

AT the 143d meeting of the society, held in the assembly hall of the Cosmos Club, Wednesday evening, April 22, 1903, a general discussion on the problem of the occurrence and storage of crude petroleum and petroleum products was opened by Dr. David T. Day in a paper entitled 'Experiments on the Diffusion of Crude Petroleum through Fuller's Earth.'

Dr. Day reviewed a series of experiments which he has been carrying on intermittently within the last five years on the changes which take place in crude oils by diffusion through various porous substances.

It was found that if crude petroleum were allowed to pass slowly through finely pulverized fuller's earth, it became separated by fractional diffusion into a series of oils differing in color and specific gravity from the original product, and representing the comparatively simple oils of which the complex crude petroleum is composed. In color the fractions varied from the dark brown or amber of the crude to the clear white of refined petroleum, and variations in specific gravity from .70 to .85 were secured.

An account was given of a series of experiments conducted by Professor Engler, to determine the nature of the changes which took place in the oil. Professor Engler's conclusion was that no chemical change whatever took place in the process of diffusion, the differences in the resulting products being entirely physical.

Experiments as to the effectiveness of various diffusion media tended to prove that the best results are invariably attained by the use

of fuller's earth. Quartz sand and amorphous silica exhibit practically no selective action. Powdered limestone was equally ineffective. Different clays show greatly differing capacity for separating the petroleum oils, the greatest effectiveness being secured as the clay approaches fuller's earth in composition and texture.

Interesting practical and scientific suggestions made by Dr. Day as a result of his experiments were, first, that the great variety in color, specific gravity, viscosity, etc., of the different Pennsylvania oils may be due to differences in amount of diffusion suffered by these oils in passing upward through Carboniferous shales from a common source, rather than to differences in original composition; and a corollary of this suggestion is that these oils and those of the Trenton district in western Ohio may have an identical original source far down in the geologic column, the present differences being due to the greater diffusion suffered by the Pennsylvania oils in passing upward through the intervening strata to the horizons in which they are now found.

An examination of the Texas oils shows that they contain considerable amounts of constituents which are most easily removed by diffusion, the conclusion being, therefore, that they are nearer the original source than the Ohio and Pennsylvania oils.

At the conclusion of Dr. Day's paper a number of geologists, among them Messrs. Hill, Eldridge, Hayes, Adams, Butts and Fuller, discussed various phases of the problem of the origin and storage of the fluid and gaseous hydrocarbons, the suggestions of Dr. Day as to the competency of fractional distillation by diffusion to account for the present differences in native oils, and the further suggestions of a common origin for many of them being regarded as of particular interest.

W. C. Mendenhall,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE PROPOSED BIOLOGICAL LABORATORY AT THE TORTUGAS.

To the Editor of Science: Since the subject of a research laboratory in or near the tropics is under discussion, I would like to have the privilege of a few comments. For the last two years I have been discussing the matter privately with a number of people who are interested in establishing a subtropical biological laboratory. This is by no means new, since an attempt was made a few years ago to carry on research work at Bimini, an island about fifty miles east of Miami. At another time the question of locating a laboratory at Miami was under discussion, and my department (biology) at the Florida Agricultural College pledged a small sum to aid in the expense of carrying on this work.

The advantages of establishing such a laboratory at Miami may be stated as follows: (1) Miami is about one degree of latitude south of the northern limits of the Antillean flora. (2) The city is located on the Biscayne Bay, a magnificent sheet of water that is always navigable to sail-boats and launches. (3) The Gulf Stream is within easy reach, being only about six or seven miles out, cat-boats and launches making one or two trips a day during the tourist season. (4) The Miami River, Little River and other streams, which drain the everglades, may be explored easily for fresh-water forms. (5) The everglades can be reached by row-boat or bicycle, macadamized roads having been built to the edge of the glades. This vast unexplored region, about which so many erroneous ideas are abroad, is now open for botanical exploration. (6) Miami is within twenty-four hours of Havana and twelve hours of Key West by steamer. Washington can be reached in thirty-six hours. (7) Launches and sail-boats may be chartered at any time at reasonable rates. (8) Miami is located south of the 26th degree of north latitude.

At the present time the tropical laboratory has only one room which can be offered to visiting biologists. This has been in almost constant use during the past year. Among those who have taken advantage of this opportunity are Professors V. M. Spalding and J. H. Comstock.

There can be no doubt as to the healthfulness of the location, and the comforts of life

are abundantly supplied. The city has a population of about 4,000, and has water works, sewer system and electric lights. There are over forty miles of macadamized roads in the best of condition for wheeling. The question of mosquitoes has been raised by one of the people discussing this matter. There are certain places in the state of Florida that are seriously infested, but at the location under discussion there is very little difficulty. The hommocks and mangrove swamps will prove to be thickly infested during the summer months, but in the clearings and on the pine land the mosquitoes are certainly not more annoying than in Iowa or Minnesota.

In a recent number of SCIENCE, one of the correspondents discusses the question of locating the laboratory in Jamaica. The location may be an ideal one from several points of view; it is, however, an out-of-the-way place, and one difficult to get to and get away from. There are practically only three lines of steamers which permit one to visit the island comfortably. These sail from Boston, New York and Philadelphia. To reach the island it would be necessary to sail from one of these ports. The average worker would lose nearly a week in getting to the laboratory. To attempt to make the trip by way of Havana and Santiago requires nearly three weeks, together with a very heavy expense account. The fact of traveling in a foreign country has some fascination about it, but has likewise its disadvantages for scientific research. We have on our southern coast a vast unexplored region which has been given only a cursory examination.

Dry Tortugas has also been under discussion. This seems to be an ideal place for solitary confinement, as one of our correspondents points out. This is all right for about six or eight hours a day, but during the other sixteen or eighteen hours it is very pleasant to have the companionship of people. Dry Tortugas would have to be reached by way of Key West. It being about sixty miles west of Key West, one would have to charter a sail-boat or a launch and pay for at least two days to make the trip, a considerable loss of time.

The majority of people who work in these research laboratories have only two or three months to put in at one time, and it becomes very important, therefore, that they should not spend twenty-five per cent. or more of this time in going to and returning from the laboratory. A laboratory located at a railway station would prove much more convenient, even if located in a field not so rich as one that would be located at a point which requires a considerable amount of traveling to be reached.

P. H. ROLFS.
U. S. DEPARTMENT OF AGRICULTURE,
TROPICAL LABORATORY, MIAMI, FLA.

TO THE EDITOR OF SCIENCE: I have read with interest the letters in SCIENCE relating to the establishment of a biological station at the Tortugas, also Professor MacBride's suggestion favoring the Bahamas and Professor Duerden's favoring Jamaica.

Will you kindly allow me to make one further suggestion? I would ask that the Isle of Pines be considered as a location for this station and for these reasons:

1. As Professor MacBride says, West Indian waters 'surpass in interest and variety of species the Mediterranean.' The fringing ceyos, bays and rivers of the Isle of Pines give areas of water at varying depths and, it seems to me, conditions unsurpassed for collecting the greatest variety of species.

2. This island, though in the tropics has, because of sea breezes, a climate agreeable for study the year round. The dry, buoyant air prevents one feeling the languor usually experienced in the tropics.

3. The fresh water supply at the Isle of Pines is unsurpassed. The fertile gardens can supply food, and lumber and building material are abundant.

4. The climate is most healthful, there being no epidemic sickness. Yellow fever, typhoid fever and malaria are unknown. Americans can live there for years in excellent health.

5. This island belongs to the United States, and it is probable that a naval coaling station is to be located there. It is easy of access.

Students then could, on the Isle of Pines, have a delightful place of residence, with all

the requisites of health and comfort and variety of scenery and exercise, while perhaps no other place could supply a greater abundance of the material for study, both marine and terrestrial.

Of course I am unable to give an opinion of value on this subject. I only ask an investigation of this Island of Pines.

J. Fred. Clarke.

'MOUNT PELEE.'

To the Editor of Science: In Science for June 5 Mr. Mark S. W. Jefferson raises a question which is of interest to those who, like myself, are studying the volcanoes of the West Indies: What shall we call the now celebrated volcano on the island of Martinique? Mr. Jefferson seems to be inclined to use the name 'Mount Pelee.'

During a stay of four weeks on the island last year and another visit of like duration this year, I heard the mountain called almost invariably 'Mont Pelé,' very rarely if at all 'La montagne Pelée.' The latter form is that employed on the charts of the island, but the former is the one most commonly used by the French in correspondence and in written descriptions, as well as in conversation, as being more compact. The general tendency among geographers now is toward using geographical names in the way in which they are employed in the region containing the geographical feature, hence it seems to me better to write the correct French 'Mont Pelé,' than the Anglicized 'Mount Pelee,' in which there is little suggestion of the true pronunciation of the name. When but one word is to be used for the mountain, the generally accepted form, 'Pelée' is convenient and is to be recommended as conforming the formal appellation of the volcano. I speak with the more feeling on this topic, because I am one of those who have helped to perpetuate the incorrect combination, 'Mt. Pelée.'

Regarding the origin of the name and its applicability to the mountain it may be remarked that the accepted explanation among Martiniquans is that the term has been derived from the ancient Carib name for the mountain. When Columbus discovered Martinique he found a Carib town at Le Carbet, nearly two miles south of the present site of St. Pierre. The Caribs were afraid to live any nearer to the volcano on account of their traditions regarding its activity; and they called it the 'bald' or treeless mountain, a name which in itself indicates traditional eruptions. Any one who has seen Mont Pelé since May 8, 1902, will grant that the mountain now merits its name.

Edmund Otis Hovey.

SHORTER ARTICLES.

ON THE LIMITS OF UNAIDED VISION.

It is generally accepted that the sixth stellar magnitude is the limit of naked-eye vison. Though observers with eyes of unusual sharpness may under favorable conditions see stars nearly an entire magnitude fainter, that this is for all practical purposes the limit may be seen from a consideration of the faintest stars given in the various star catalogues and uranometriæ devoted to naked-eye stars. The average magnitude on the scale of the Harvard photometry of the faintest stars visible in several of these catalogues is as follows (*H. C. O. Annals*, Vol. XIV., Part II.):

Ptolemy's Almagest	5.38 M.
Sûfi	5.64 "
Argelander, Uranometria Nova..	5.74 "
Heis, Atlas Cœlestis Novus.....	6.06 "
Houzeau, Uranométrie Générale.	6.40 "
Gould, Uranometria Argentina..	6.71 "

Argelander states that his sixth magnitude comprised stars as faint as he could make out at Bonn; his eye, according to his own estimate, was of moderate sharpness. The faintest class of Houzeau comprised those stars which, under favorable conditions, could not be seen continuously, but only at intervals. Gould found in the clear atmosphere of Cordoba that on very good nights observers of ordinary vision might go even below his seventh magnitude (6.71 M. Harvard phot.), and attributes it mainly to the advantage given by the altitude of the observatory. Several of the observers at the Lick Observatory have, under the most favorable condi-

tions, seen stars well down toward the seventh magnitude.

The invisibility of stars of the seventh magnitude or slightly fainter is due mainly to the amount of light given by the background of the sky, even on the clearest nights and in regions well removed from the Milky Way (*cf.* papers by Professor Simon Newcomb, *Astroph. Jour.*, December, 1901; Dr. S. D. Townley, *Pub. A. S. P.*, No. 88, and G. J. Burns, *Astroph. Jour.*, October, 1902). At Professor Newcomb's suggestion Director Campbell has asked me to find my own limit of naked-eye vision, having given as artificial aids the direction of the star, and the screening off of the light of the sky.

Two blackened screens were attached to the twelve-inch telescope at a distance apart of 178 inches. The rear screen was pierced with an aperture half an inch in diameter and that at the object glass with one of one quarter of an inch. These apertures were so aligned that when a star was seen centrally through them it would be found at the intersection of the cross-wires of the three-inch finder. A movement of two or three minutes of arc was sufficient to carry the star out of the field thus formed.

The method of observation adopted was to clamp the telescope at the proper declination for the selected star. It was then swept slowly in right ascension with the eye at the aperture till the star was picked up. The position of the star was then noted in the finder and if not more than a minute or two of arc from the intersection of the cross-wires the observation was considered successful. Several such trials were made on each star.

Eleven stars were observed on three nights, of which only the last could be called a very clear night. The magnitudes of the stars employed ranged from 6.42 to 8.5. It was found that up to and including magnitude 8.0 the stars could be certainly seen in every instance, though in no sense easy objects. Stars of magnitudes 8.1, 8.2, 8.3 and 8.5 were seen with great difficulty and with occasional failures, generally when the eye was tired from the strain of searching for these very faint objects.

The contrast between the almost perfect darkness of the object glass screen and the sky immediately around it, as seen through the rear aperture, was very marked. It seems evident that for the observation of such faint objects without telescopic aid the screening off of the light of the sky is more important than the concentration of the vision in a definite direction as afforded by the use of the apertures.

HEBER D. CURTIS.

LICK OBSERVATORY,
April 22, 1903.

A MODIFICATION IN MEASURING CRANIAL CAPACITY.

ONE of the most important, but at the same time rather difficult and tiresome, manipulations in anthropometry is the measuring of cranial capacity. The importance of the measurement lies mainly in that it gives us the volume, as well as a fair basis for the calculation of the weight, of the brain, both of which data are very valuable in racial comparison, and, so far as most of the more primitive races of people are concerned, are quite impossible to be secured in any other manner.

It is plain that a procedure of such importance should be brought to the utmost possible simplicity and perfection, and so regulated that the capacity measurements could be utilized with full safety and universally. This sentiment was undoubtedly common to all the practical workers in physical anthropology up to the present day, and the results have been an invention of many more or less related methods for measuring cranial capacity, and a gradual approach to an ultimate, generally adoptable, procedure, under the circumstances the nearest possible to perfection. It is in connection with this very desirable, ultimate method, the main points of which are already well understood, that there is still a place for some modification, and one such will be described in this paper. In the first place, however, it is advisable to give a few explanatory notes as to the various procedures in general use.

The many methods of measuring cranial

capacity can be segregated into five groups, namely:

1. The skull is made impermeable and then filled with some liquid, preferably water, which is then weighed or measured; or the water is forced into a thin rubber bag until it fills with this the entire skull cavity, after which the liquid is measured. These methods, employed by Broca, Schmidt, Matthews, etc., yield good results, but are too complicated or tedious for ordinary use.

2. The skull is filled with sand or other substances, and this is weighed, the result giving a basis for calculating the capacity. This method, used especially by some American anthropologists of the last century, was not sufficiently accurate, and soon became obsolete.

3. The skull is filled with small, rounded seeds, beads, shot or other substance, and the contents are then measured (Tiedemann, Busk, Flower, etc.). The filling or the measuring (or both) is aided by certain manipulations (tilting, tapping, etc.), but, except the measuring vessels, no implements are required. The method in its numerous modifications is comparatively easy and has other advantages, but the results are mostly not as accurate as desirable.

4. The method invented and regulated by and named after Broca. In this procedure the skull is packed with shot, which is then measured; but both the filling and measuring are aided by certain implements, and every step of the procedure follows definite rules. Among the implements used appears a funnel of certain dimensions, which controls the flow of the shot. The method gives steady results, but can not be used with frail skulls, and the capacity obtained is always larger than actual, the proportion growing with the size of the skull.

3. The Welcker's method.* In this procedure, which is the outgrowth of the majority of those mentioned, but more directly of that of Broca, the most important part is delegated to the funnel, which, by its size, controls the measuring of the contents of the skull. The mode of filling the skull, so long as efficient and uniform, is immaterial; all that is required is that each worker should, with the aid of a standard skull, find the exact size of the funnel necessary to give him, in measuring, the correct result with his particular method and substance used for the filling of the skull. Any rounded seed or substance can be employed for the filling, as it is possible to completely fill the cranial cavity without using the process of jamming, such as that used by Broca; this allows the most fragile skull to be measured without any injury. Welcker advocated a funnel large enough to receive all the contents of the skull. The contents of the properly filled skull are emptied into a separate vessel and then 'with one movement is versed into the funnel,' which is open (not provided with any stopper) and held in position vertically and centrally above the graduated receiving vessel. Each new series of measurements is controlled by the standard skull.

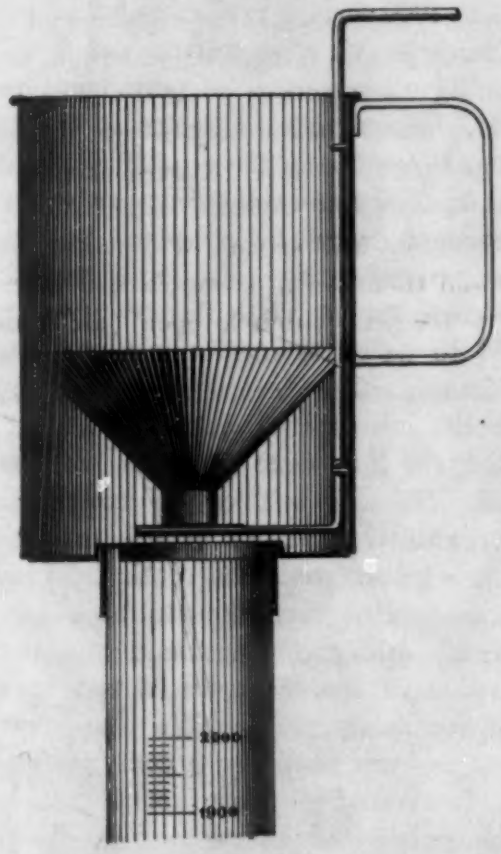
There can be no doubt that this last is the most advanced and preferable method, and the one which, a little more perfected, presents the best claim for universal adoption.

Experimenting for nearly six years, at the American Museum of Natural History, with the various earlier procedures of measuring cranial capacity, I arrived, a little over two years ago and practically independently, at a method which in principle is identical with that of Welcker, but is carried on with a few further helping details which deserve being mentioned.

Starting, as Welcker, with the laws concerning the flow of solid substances, etc., as laid down by Broca, and with Broca's implements and a standard skull, I was soon able to satisfy myself that: (*a*) The same substance poured through the same funnel with the same rapidity will always give the same, but with different rapidity will give differing, measures; and that (*b*) each different substance that can be utilized for the measurement of cranial ca-

* *Arch. f. Anthrop.*, Bd. XVI., S. 1 et seq. E. Schmidt, 'Anthropologische Methoden,' pp. 217–219. Recently a modification of the instruments with a form of a funnel stopper has been proposed by E. Landau, *Intern. Centralblatt f. Anthrop.*, etc., 1903, I., pp. 3–7.

pacity, flowing through a definite size funnel and with regulated rapidity, will give different results from those given by any other substance flowing through the same funnel and with equally regulated rapidity. (c) Given the same regulation of rapidity of the flow, there can be obtained, through the proper selection of funnels of different diameter, any measurement, ranging between the minimum and maximum of a substance of medium weight and size, by all the solid substances employable for filling the cranial cavity.



Without going into details, I may state that I obtained a very efficient regulation of the flow by adding to the funnel a movable stopper.* By doing this I found by many practical demonstrations that it becomes immaterial as to with what rapidity, or in what manner, the funnel is filled before opening the stopper. This removes at once all source of error connected with the emptying of the cranial contents, and allows us to dispense with the extra vessel used in measuring the cranial contents in Welcker's procedure. With the funnel closed, the cranial contents are poured into it entirely at the convenience of the measurer.

In 1901 I had constructed, mainly on the basis of Broca's, a special apparatus, of which a cut is inserted, and with this I have worked since with much ease and with entire satisfaction. My favorite mode of filling the skull is that used by Flower. To measure the contents, they are emptied directly, in any way desired, into a combination of a zinc vessel (higher than, but otherwise similar to, the standard Broca's double litre) and a removable funnel of 45° dip, with 15 mm. high vertical section, which, for my purpose (using old, dry mustard seed), is 20 mm. in diameter. Immediately below the funnel is a movable disk which acts as its stopper. The disk is attached to a rod which rises along the side of the vessel and above its border, and ends in a lever; by using this lever the disk closes or opens the funnel. A number of extra funnels, of the same dip but of different sizes, are provided, from which to choose if another substance than mustard seed is used for the filling. The vessel with the cranial contents is placed on the top of a 2,000-c.c. graduated glass tube (such as used by Ranke), which is fixed in a vertical position. The zinc vessel is provided with a groove in its bottom which exactly fits the border of the glass, the opening of the funnel being central. Then the lever is rapidly pushed to either side, opening the funnel at once and completely, and the flow left to itself; the level which the seed reaches (determined simply by the eye or, preferably, by the careful aid, without any shocks or pressure, of a niveau finder, such as comes with Ranke's tube) is the skull capacity. Thus the measuring part of the capacity determination is entirely reduced to a mechanical one, which not only makes it very easy, but eliminates from it all source of error due to personal equation. All that a student needs to learn is some method by which a complete and uniform filling of the skull can be effected, and then, working with the aid of the standard skull, choose the proper funnel; the rest is

* Landau's stopper differs in kind, but is apparently allied in purpose.

controlled. The results, always with the condition that the proper use is made of the standard skull, are as uniform and as near the reality as can be reasonably hoped for.

The apparatus I use is not made for the market, but it should not be difficult for any one sufficiently interested to have it constructed, following the given directions.

Alës Hrdlicka.

U. S. National Museum.

NEW DEPARTURES IN THE CONCILIUM BIBLIOGRAPHICUM. II.—THE SUPPLEMENTARY BIBLIOGRAPHY.

This portion of the great catalogue exists primarily in manuscript form. As fast, however, as the demand for any part becomes great enough the cards are duplicated by a new lithographic process. Should the demand become still greater, typography would be resorted to. This ought certainly to be the case for the American section.

The references consist of such entries as can not for practical reasons be admitted into the general bibliography. The price of any given collection of cards is double that charged for the same number of cards taken from the main bibliography, ranging thus from half a cent to two cents a card.

1. *New Genera, Species, Subspecies, etc.*—As was shown in the first part of the present article, the Concilium adopted several years ago the uniform practice of reading, or at least perusing, the text of every publication entered in the bibliography. In this way the descriptions of all families, subfamilies, genera, subgenera, species, subspecies, etc., described in zoology passed before our eyes. It seemed under such circumstances a great pity that the information thus acquired should not be placed at the service of the zoological world. A beginning was made by referring to every new species under the appropriate genus name in connection with the printed cards. This has been made a permanent feature of the card catalogue, and no card is now issued which does not bring references to all new species described and to all new names introduced by the author. Many zoological memoirs contain descriptions of several hundred new species; but we have never wavered in our decision to record everything, even though double and triple cards were required.

The value of these entries is very great; but we have, of course, not been able to cite every species in full, nor to state the locality from which it came. Nor were the printed cards giving references to the new species available for an adequate catalogue of new species; for, in most cases, species from many different genera being described in a single publication these were recorded on a single card. In 1900 certain preliminary experiments were made in view of testing the possibility of placing all novelties on permanent record, so that, for example, a zoologist turning to the genus *Carabus* in the year 1950 might find before him in convenient form an exhaustive catalogue of every new subgenus, species and of every new name introduced under that genus since 1901. The value of such a record seemed to us quite inestimable and one can well imagine the feverish impatience with which the outcome of our experiments were awaited, for they were to decide whether this gigantic task lay within the possibilities of our organization. The experiments showed that the labor would in truth be immense. We also had to face the stubborn fact that we were working for posterity and that the full value of the work would not be appreciated for many years. We also knew that the entire work would be a complete financial loss. In view of all these facts it was, indeed, a bold decision which we took on January 1, 1901, when we began recording each novelty on a special card. The work is now progressing well, and if the Concilium be adequately supported, will never be abandoned.

The collection of references to such a genus as *Carabus* will not be the only facility which the Concilium will offer to the student coming to it in 1950. If a person is then desirous of studying the fauna of Bolivia, my successor in office will first show him the main printed bibliography, comprising at that date some 200 to 300 entries; he will then lead the visitor to a great cabinet of 72 drawers devoted to the new species described from South Amer-

ica. At the present rate of publication, three library bureau drawers would, fifty years hence, be devoted to Bolivia. The 2,500 cards would not, however, be indiscriminately arranged. One drawer would be devoted to arthropods. The greater part of the drawer would be filled with references to the insects, readily recognizable by the appropriate symbol of the decimal classification. A rather large group of cards would follow the guide card Coleoptera. Relatively smaller packets would refer to the primary subdivisions of the Coleoptera. I do not seriously believe that a representative of the genus *Carabus* will have appeared in Bolivia; but I trust that the references to Felsche's species of *Pinotus* will still meet the eye of the visitor of 1950 and that he will respect the self-sacrifice that made the inauguration of the work possible.

Multiple entry is the feature of the catalogue of new species as of the printed bibliography. Thus, in the paleontological part, there is not merely a division for dinosaurs, there is also an exhaustive treatment of the fossil fauna of Kansas gathered together under the heading Kansas, and a reference to all discoveries of Cretaceous animals under the appropriate stratigraphic heading.

Most of the work that we are here undertaking has never been attempted before; but our experience has shown that, in such places as our activity runs parallel to other recording agencies, we find in the latter so numerous omissions that the necessity of a more perfect organization of the work is most apparent. Even in regard to genera, a cursory comparison showed omissions aggregating a hundred or more, while, in regard to species and subspecies, we are sure that many hundred are recorded in the catalogue of the Concilium which are elsewhere quite overlooked. Omissions, of course, occur in our lists, but, again, we know just where the gaps lie and can make them good as soon as we can obtain access to each publication which we had not hitherto been able to excerpt.

The two years' experience has shown us, furthermore, how impossible it is for the individual worker to avoid giving preoccupied names. As soon as we detect such errors we inform the author and suggest a change. Often the same name is chosen by two authors within a few weeks of each other, so that the entry in our record with date is highly important. In regard to genera the case is most disturbing. How can an ornithologist describing a new genus of birds be sure that his name has not been used by some paleontologist describing a fossil sponge? No such universal knowledge of the literature can be expected of any worker. For this reason, the Concilium is anxious to issue at frequent intervals, perhaps yearly, with cumulative five-year and twenty-five-year indexes, a cheap concise list of genus names that have been proposed. This work would supplement the 'Nomenclators' of Agassiz, Marshall and Scudder and the work recently published by the Zoological Society of London.

The following entry may serve to illustrate the arrangement of the text of our record of new species:

57.98 NESODYNERUS (96.9)
obtabilis n. sp.
Perkins 1901*a*.
Entom. Monthly Mag. (2), Vol. 12, p. 267.

57.98 is the group number for Vespidæ, (96.9) for Hawaii. A second entry is made under (96.9) as primary division.

2. *Minor Notes.*—Many local journals of natural history, *e. g., Entomologist, Auk,* etc., publish, often in small print, numerous notes on captures, isolated observations of habits, records of trifling color variations and so forth. Ever since the foundation of the Concilium it has been a burning problem how to deal with these notes. It is out of the question to neglect them, for they may contain changes of nomenclature which by the rule of priority might become decisive of the proper name of an animal. Thus we may find new genera of fishes actually introduced for the first time in the editorial talks on recent literature appearing in the *American Naturalist!* No form of publication can be too trivial for a conscientious bibliographer, so long as the law of absolute priority forms the basis of our nomenclature.

In regard to these minor notes, the bibliographies in pamphlet form have a vast ad-

vantage over a card bibliography. No one need hesitate long to sacrifice two lines of print even to an almost worthless publication. But when it is a case of publishing a complete bibliographical card it becomes a most serious matter. For many years the Concilium tried various subterfuges; it issued many cards that seemed scarcely worthy of notice; it then experimented with printing the references on gummed paper to be pasted on cards by the subscriber, if desired. It also tried holding back such references till the end of the year and then publishing a dozen or more entries on a single card.

None of these means was successful. Finally, in 1902, a great catalogue of manuscript entries was founded. Such entries being omitted from the printed bibliography, the total number of cards sent to subscribers will be reduced annually by a thousand or more. The new cards are similar to the ordinary bibliographical cards in every respect, save that instead of being printed they are in manuscript. They can, however, be supplied when desired. Thus a subscriber in California will not be burdened by innumerable tales of beetles found in the county of Kent, England. For the inhabitant of Kent the note may have value, for the Californian it is rather superfluous. In future the Kent entomologists can receive the reference if they care for it; but the Californian ornithologist need not. This new departure means a loss to the Concilium of several hundred dollars annually. The maintenance of the manuscript catalogue is costly and the income from the printed card catalogue is reduced by leaving out such notes. It is merely a case of what I may conscientiously declare to have been the uniform policy of the Concilium, that of never deviating a particle from its disinterested aims. Every innovation of recent years has been attended with financial loss; but never have we faltered in assuming it. We have perhaps often imperiled the very existence of the work by such scruples, but at the same time we have, I believe, won the unqualified approval of every one who has taken the pains to examine closely our work and can with justice appeal for support to those who have the interests of science at heart. I am confident that the disaster which the abandonment of the work would entail is not a danger which is seriously threatened. The immediate needs of the institute seem to us great; but they are only so in comparison with the modest means which have thus far succeeded in keeping the enterprise alive. A debt of $4,000 ought not to burden indefinitely the work. $3,500 for new machinery ought not to be a hopelessly large sum to secure. A yearly grant from an American source equal to that offered by little Switzerland ($1,500) seems least of all exaggerated. And yet this is all that is needed to inaugurate a period of prosperity and work without preoccupations of a financial character. Doubtless new possibilities and new needs would open as the years passed; but the present program could be fully carried out with the support that I have sketched.

Herbert Haviland Field.

*REPORT OF THE ICHTHYOLOGICAL RESEARCH COMMITTEE.**

We understand that the committee appointed by the Board of Trade in August last year 'to inquire and report as to the best means by which the State or local authorities can assist scientific research as applied to problems affecting the fisheries of Great Britain and Ireland, and, in particular, whether the object in view would be best attained by the creation of one central body or department acting for England, Scotland, and Ireland, or by means of separate departments or agencies in each of the three countries,' have come to the conclusion that, while no sufficient reason has been adduced for suggesting any changes as to the central authority for conducting scientific fishery investigations in Scotland and Ireland, it is desirable that the functions of the Fisheries and Harbor Department of the Board of Trade, which is the central authority for England, should be considerably enlarged. They recommend, therefore, that the Board of Trade should have power not only to delegate to any satisfactory fishery authority the conduct of such fishery

* From the London *Times*.

investigations as the latter body are willing and able to carry out, but also themselves to conduct investigations; and, further, that the local fishery authorities and the authorities or bodies who conduct or contribute to the expenses of such investigations should be represented on a Central Fishery Council at the Board of Trade, which council should have general control over all such investigations.

To give practical effect to this scheme the committee recommends the constitution at the Board of Trade of a Fishery Council for England, consisting of one expert and one administrative member representing each of the three coasts (E., S., and W.), together with such official representative or representatives as the department may nominate; the duty of this council being to meet periodically, to formulate schemes of investigation, to make recommendations as regards Governments grants, to report on the knowledge acquired by the researches made, and, generally, to exercise control over the investigations. In order to secure uniformity of action between scientific bodies at work in the seas surrounding the United Kingdom, and to prevent overlapping of areas of research, the committee suggest that there should be a conference consisting of representatives of the three central authorities to this extent only. The committee propose that statutory powers should be given to the local sea fisheries committees to expend money on fishery research; and, recognizing that on the east coast of England (except to a small extent in Northumberland) there are no local fisheries committees contributing to the knowledge of fishery problems, they recommend that the Fishery Council for England should itself conduct investigations on that coast. The committee think, however, that, if possible, the expenses of such investigations should be partly borne by those who are pecuniarily interested in them.

Another point emphasized in the report is the desirability of making provision at the principal fishing ports of the United Kingdom for the collection of statistics on the largest scale practicable for the purpose of ascertaining the particulars of the fish landed and the place where they are caught. To achieve this end the committee suggests that arrangements should be made for the payment of masters of fishing vessels for filling up returns, and for the engagement of a staff of trained assistants to deal with these returns and with the samples landed and selected for examination. In addition to the research vessels possessed by the central authority in Ireland and by the Marine Biological Association at Plymouth, three special steamers should, the committee think, be provided to study definite sea areas, one to work on the east coast, a second along the west coast of England, and a third to replace the *Garland* on the coasts of Scotland. If the plan formulated by the committee finds approval, each of these steamers will work in connection with a biological laboratory; and each laboratory (in addition to those already established in Scotland and Ireland) will have three biological assistants, while another assistant will be stationed at the office of the central authority in each country. The committee explain that the laboratories at Liverpool and Port Erin (Isle of Man) would meet the wants of the west coast, while that at Plymouth would suffice for the south coast. It would be necessary, however, to establish a new laboratory at some central point on the east coast of England.

Finally, the committee expresses the view that benefit would be derived from the establishment of a National Fishery Museum, which had best be placed at a great fishing center, such, for example, as Grimsby. In this museum might be exhibited such of the specimens of the Buckland collection as are worth preserving; and, it is added, "perhaps it would be found possible to apply Mr. Buckland's monetary bequest for the purpose of such a museum, which might very appropriately be united with the proposed laboratory for the east coast." The original chairman of the committee was Sir Herbert Maxwell, M.P., but he resigned the post last May, since when the inquiry has proceeded under the presidency of Sir Colin Campbell Scott-Moncrieff, whose name appears first amongst the signatories to the report.

OPPORTUNITY FOR GEOLOGICAL RESEARCH IN HONDURAS.

THE great excavations and tremendous cuttings into and through the mountains along the line of the new highway from Tegucigalpa to the south coast have laid bare large parts of these mountains.

I desire to call the attention of geologists and students of geology to this rare opportunity for research in this field of science. It is of especial interest to students in volcanic formations and action. Calcareous deposits abound, and metamorphic formations may be studied minutely. There are excellent exhibits of shale formation as well as of tufa and other igneous conglomerates. Pumiceous deposits and volcanic sand present a fine chance for study at first hand.

These cuttings have entailed an expense of many thousand dollars, and geologists may now profit by the result without any greater expense than that of travel and living while here.

Amapala, Honduras, may be reached by the steamers of the Pacific Mail Line from San Francisco, or from Panama (connecting with Panama R. R. S. S. Co.). The entire expense, including that while in Honduras, should not exceed three hundred dollars.

It would be advisable to make use of tents and camping paraphernalia, as the accommodations for strangers are very crude as well as limited.

Since vegetation is luxurious and of rapid growth in these countries, I suggest that those intending to make a study of these formations do so at once, as the surface of these now bare cuttings, excavations and slides will, in not many months, be overgrown with tropical flora.

ALFRED K. MOE,
U. S. Consul.

TEGUCIGALPA, HONDURAS,

THE NEW YORK COLLEGE OF FORESTRY.

THE trustees of Cornell University at their meeting on June 17 passed the following resolution:

"Owing to the failure of the state to appropriate means for the support of the College of Forestry, established by the state at Cornell University, resolved, that instruction in that college be, and the same is hereby, suspended until ways and means are provided therefor by this state, and that all appointments to the instruction force, including the appointment of the director, be vacated."

In his report to the trustees President Schurman writes as follows:

"The administration of the New York State College of Forestry was undertaken by Cornell University at the instance of the state, the university having neither asked for the establishment of such an institution nor been consulted concerning the terms of the act under which it was organized. But when the legislature framed and the governor approved a bill inviting Cornell University to conduct a great experiment in forestry in the Adirondacks in connection with a State College of Forestry, to be established at Ithaca, the university, in its loyal desire to cooperate with the state in this scientific enterprise, accepted the duty imposed by the act and addressed itself to the task with good faith, diligence, and an earnest determination to carry out the purpose of the state as expressed in the terms of the act itself.

"The first and all essential step was to secure an expert into whose hands, under the general supervision of the trustees, the work might be committed. The university congratulated itself on securing the services of a gentleman who had been thoroughly trained both on the theoretical and practical sides in European forestry, who had lived many years in the United States, and who, after successful experience as a forester for private parties (among whom the late Abram S. Hewitt strongly testified to his merits and success), had for some years held the foremost position in forestry in the United States, namely, that of chief of the Division of Forestry. From this office Dr. Bernhard Eduard Fernow came to the position of Director of the New York State College of Forestry. He outlined

a plan for the conducting of scientific forestry in the tract of 30,000 acres in the Adirondacks which the state had assigned to the College of Forestry for that purpose. The plan grew out of the actual condition of the tract in question. It was a scheme to substitute valuable soft woods for old and rotten hard woods. This meant denudation and replanting. But there is a general prejudice against cutting even old trees and an impatience to wait as long as fifty years for new ones to take their place. Both feelings have been invoked by critics of Director Fernow's work in the Adirondacks. And without going into further detail, the result now is that the state, speaking through its organized authority, desires to have the work stopped. The university stands by its expert. But the university has not the means, even if it had the power, in the absence of state appropriation, to carry on the work of the College of Forestry.

"What is to be done under these circumstances? The President believes that the wishes of the state in regard to the Adirondacks tract which it has placed in charge of the college should be observed as soon as these wishes can be officially ascertained. All that the university need insist upon is indemnity against liability assumed as agent of the state in the contract with the Brooklyn Cooperage Company. If the state, on mature consideration, disapproves of the plan of forestry adopted by Director Fernow, the university has no interest in attempting to force that plan upon the state, however excellent it may be in itself or however extensively it may be practised in Europe or America. Not a cent of state money has inured to the benefit of Cornell University, though the state work in forestry has entailed heavy burdens and anxieties upon the president, treasurer and trustees. It is a hardship to deprive so many students of the opportunity of completing their course, and a matter of regret that the first college of forestry in the United States should be suspended or discontinued, but the action of the state authorities seems to give the trustees no alternative."

SCIENTIFIC NOTES AND NEWS.

DR. CARL GEGENBAUER, the eminent anatomist, since 1863 professor at Heidelberg, died on June 15, at the age of seventy-seven years.

A MONUMENT in honor of Pasteur was unveiled on June 7 at Chartres, near which Pasteur carried on his experiments on anthrax. Addresses were made by M. Chauveau, representing the Paris Academy of Sciences and M. Chamberland, representing the Pasteur Institute. The monument is by Dr. Paul Richer, who is both a sculptor and physician.

PROFESSOR J. H. VAN'T HOFF and Professor Robert Koch, of Berlin, have been elected honorary members of the Vienna Academy of Sciences, and Sir William Ramsay and Professor Georg von Neumayer corresponding members.

M. MUNIER CHALMAS has been elected a member of the Paris Academy of Sciences in the section of mineralogy in the room of the late M. Hautefeuille. Professor H. A. Lorentz, of Leiden, has been elected a correspondent of the academy in the section of physics.

THE HONORABLE ARTHUR BALFOUR, the British premier, has accepted the presidency of the British Association for the meeting to be held in Cambridge in 1904.

DR. D. C. GILMAN, president of the Carnegie Institution, gave the address at the recent convocation at the University of Chicago. The university conferred its LL.D. on Dr. Nicholas Murray Butler, president of Columbia University.

TUFTS COLLEGE has conferred its LL.D. on Dr. Carroll D. Wright, U. S. Commissioner of Labor.

RUTGERS COLLEGE has conferred the degree of LL.D. on Dr. C. M. Ellenwood, president of the Cooper Medical School, San Francisco, and the degree of D.Sc. on Joseph F. Hills, professor of agricultural chemistry in the University of Vermont.

THE degree of Sc.D. was conferred by the Western University of Pennsylvania upon Mr. William Harris Ashmead, the curator of the entomological collections of the United

States National Museum, in recognition of his distinguished contributions to the literature of hymenopterology.

The alumni of the College of the City of New York gave a dinner on June 15 to Professor Alfred G. Compton, acting-president of the college and professor of mathematics. Professor Compton was a member of the first graduating class in 1853, and has for fifty years served the college as instructor and professor.

The Observatory Syndicate of Cambridge University recommends that a pension of £200 per annum be granted to Mr. Andrew Graham, M.A., on his retirement from the position of chief assistant at the observatory, which office he has held for a period of thirty-nine years. Mr. Graham began his work in astronomy at Mr. Cooper's observatory in Markree, county Sligo, in 1842.

Mr. George Whitehouse, engineer-in-chief of the Uganda Railway, has been knighted.

The Harveian Lectures of the Harveian Society of London, will be delivered by Dr. D. B. Lees, next November, his subject being the treatment of some acute visceral inflammations.

Professor H. L. Fairchild, secretary of the Geological Society, sails for Europe on June 27. He will attend the International Geological Congress in Vienna, the last of August, and join the excursion through the Austrian Alps. The time previous to the congress he will spend in geological study in Italy and Switzerland.

Dr. F. H. Herrick, professor of zoology at Western Reserve University, will spend next year abroad.

Dr. Howard S. Anders, instructor in physical diagnosis of chest diseases at the Medico-Chirurgical College, Philadelphia, has been re-elected president of the Pennsylvania Society for the Prevention of Tuberculosis, which meets in the Academy of Natural Science in Philadelphia.

Dr. Elihu Thomson has been appointed president of the committee of organization of the International Congress of Electricity, which meets at St. Louis in the week beginning September 12, 1904. The other members of the committee are: vice-presidents, Professor H. S. Carhart, C. F. Scott, Professor W. E. Goldsborough, Dr. W. S. Stratton; general secretary, Dr. A. E. Kennelly; treasurer, W. D. Weaver; advisory committee, B. J. Arnold, B. A. Behrend, C. S. Bradley, J. J. Carty, A. H. Cowles, Professor F. B. Crocker, Dr. L. Duncan, H. L. Doherty, Professor R. A. Fessenden, W. J. Hammer, C. Hering, L. B. Stillwell, C. P. Mathews, R. D. Mershon, K. B. Miller, Dr. W. J. Morton, Dr. E. L. Nichols, Professor R. B. Owens, Dr. F. A. C. Perrine, Professor M. I. Pupin, Professor J. W. Richards, Professor H. J. Ryan, William Stanley, Professor C. P. Steinmetz and A. J. Wurts.

On June 15 Professor G. Jesup, from 1877 to 1899 professor of botany in Dartmouth College, died in Hanover, N. H.

We regret also to announce the deaths of Mr. Alfred Haviland, known for his work on the geographical distribution of disease in Great Britain, at the age of seventy-eight years; of M. Eugen Demarcay, the French chemist, at the age of fifty-one years; of Dr. Stanislao Vecchi, professor of geometry at the University of Parma; of Dr. Dirk Huizinga, professor of physiology at the University of Groningen, and C. L. J. X. de la Vallée Poussin, professor of mineralogy and geology at the University of Löwen.

Reuter's Agency has received the following particulars of the operations of the Danish literary expedition, which consists of M. Mylius-Erichsen, the author; Count Herald Moltke, the painter; Dr. Bertelsen, and a student, M. Knud Ramassen. Last summer they made a voyage in boats along the west coast of Greenland from the colony of Godthaab to the colony of Jacobshavn, where the expedition wintered. In February the expedition started on sledges drawn by dogs for Upernivik (lat. 73 deg. north), the most northerly Danish settlement in West Greenland, which was reached in March. By March 24 the preparations for leaving Upernivik were complete, and some members of the ex-

pedition were about to proceed in a northerly direction along the coast with the intention of reaching Cape York by way of Melville Bay, the shores of which are quite uninhabited, and which have never yet been explored. The object of the journey was to study the tribes of Esquimaux. Dr. Bertelsen was starting southwards through the Danish districts of Umnak, Godthavn and Egedesminde in order to collect material for his book on the diseases of Greenland. He purposed meeting the Cape York expedition on its way home in South Greenland this summer. Several months' provisions for six men and one month's food for six teams of dogs (ten or twelve dogs to a team) were deposited last summer at the most northern point of the Danish coast. All the members of the expedition were well when the letters left.

A CABLEGRAM from Cape Colony to the daily papers says: The German Antarctic steamer *Gauss* has arrived here and will remain about three weeks to refit and then will proceed homeward. The vessel shows outward signs of her experiences in the ice. The expedition has been a great success and not a single casualty occurred among those on board throughout her stay in the Antarctic regions. After sailing from Cape Town, December 7, 1901, the *Gauss* called at Kerguelen Island, where a party was landed. The vessel reached floating ice on February 14, 1902, and was icebound on February 22. The expedition discovered a new land, which they named Emperor William II. Land. It was covered with ice, with the exception of an inactive volcano. The expedition was icebound here for almost a year, the ship being fast in pack ice. The crew went into winter quarters, and many scientific investigations were carried out during this period. Several expeditions with dogs and sleighs left the winter quarters, but found the season too advanced, and their progress was hampered by fearful snow storms and darkness. The *Gauss* made her way out of the ice with northward flowing currents, and, leaving the ice April 8 of this year, she proceeded to Durban, passing Kerguelen Island and calling at St. Paul and New Amsterdam Islands. The expedition enjoyed good health, and there was no sickness, accident or death among its members. Professor Drygalski speaks in the highest terms of the vessel both at sea and in the ice and as regards its equipment. There were enough provisions on board to last the expedition another two years. There was no trouble with the dog teams. The results of the expeditions are briefly: The discovery of a new land in the polar circle and many special investigations. Specimens will be sent on ahead to Berlin. The expedition did not sight the British Antarctic expedition steamer *Discovery,* now icebound in the Antarctic regions, nor the ship *Morning,* which was sent to the *Discovery's* assistance.

THE Philadelphia College of Physicians has passed a resolution requesting its fellows to subscribe for the *Index Medicus,* published by the Carnegie Institution. The Carnegie Institution has appropriated $10,000 annually for its support, but this sum and the subscriptions so far received will not suffice.

THE fourteenth annual meeting of the Museums Association will be held in Aberdeen, Scotland, during the week beginning July 13, under the presidency of Dr. F. A. Bather, of the British Museum, who opens the conference with an address at 10 A.M., on Tuesday, July 14. Meetings for the reading and discussion of papers will occupy the mornings of Tuesday, Wednesday and Thursday, while there have been arranged excursions to Balmoral and Dundee, visits to the Art Gallery and Museums of the city, and various social festivities. A special attempt is being made to induce museum officials from the continent of Europe to attend the meeting, and it is particularly hoped that some American visitors to Great Britain may find it possible to be present. Some museum curators may be passing through on their way to the International Geological Congress at Vienna, and we are requested to state that the presence of those or any others interested in museum questions would be warmly welcomed at Aberdeen. Any who propose to avail themselves of the invitation should, if possible, communicate beforehand with the secretary of the

association, Mr. E. Howarth, Public Museum, Sheffield, England.

The English electrochemical society, to the steps for the organization of which we have already referred, will be known as the Faraday Society, and will hold its first meeting on June 30.

The Lake Laboratory buildings at Cedar Point, Sandusky, Ohio, will be formally opened on July 2. Addresses will be made by Professor Herbert Osborn, director of the laboratory and professor of zoology in the Ohio State University, by Professor C. J. Herrick, president of the Ohio Academy of Sciences and professor of zoology at Denison University, and others.

The whaling ship *Gjoa,* with an expedition under the command of Captain Ammundsen, has left Christiana to study the conditions about the magnetic North Pole.

Reuter's Agency is informed of the arrival on June 11 at Obbo, to the southeast of Gondokoro, of Major Powell-Cotton, Northumberland Fusiliers, who for the past year has been traveling in Central Africa. When last heard of he had been studying the cave dwellers at Mount Elgon and was proceeding towards the Upper Nile. He then expected to reach Wadelai in February.

Nature reports that in the House of Commons Mr. Austen Chamberlain, speaking on the vote for the telegraph services, referred at some length to the relations between the postoffice and the Marconi Wireless Telegraph Co. He said that the postoffice had no desire to check the progress of wireless telegraphy, nor could they have done so had they wished, as their monopoly did not extend beyond the three-mile limit. The Marconi Co. had, however, asked for too much; in the first instance they asked to be given a permanent and exclusive right to work wireless telegraphy in this country, which he could not grant, especially after the postoffice's experience with the telephone system. He had, however, granted them a private wire to Poldhu on the ordinary terms as soon as they asked for it, but before undertaking to act as their agents for the collection of messages, as was done for the cable companies, the postoffice required that certain conditions should be fulfilled in order to safeguard the admiralty, and also asked that their experts should be satisfied that the company were able to carry on their business and transmit messages across the Atlantic commercially. He was still waiting an answer to this request, which was made last March.

The *Experiment Station Record* states that the legislature of Hawaii at its recent regular session provided for a reorganization of the office of the commissioner of agriculture by placing the duties of that office under the control of a non-salaried board of five commissioners. The new law defines the duties of the board and provides for the enforcement of its regulations. Under the new arrangement particular attention of the board is given to forestry, entomology and inspection of plants, fruits, etc., to prevent the admission of injurious fungi and insects. For this work paid superintendents and assistants are provided. For the development of general agriculture, cooperation with the experiment station established by the U. S. Department of Agriculture is to be sought.

We learn from the New York *Times* that as the result of plans that have been developed since early in the spring the American Museum of Natural History has arranged to loan to the biology departments of as many of the public schools of Greater New York as may make proper application collections of invertebrate specimens for use in connection with the school biological work. It was found that two sets of collections could be prepared, one, known as the duplicate collection, consisting of about forty-five specimens, covering between thirty and forty species and illustrative of general characteristics, and the other, a specialized collection of from one hundred to one hundred and fifty specimens, collected and arranged with a view to showing typical forms of different species, and whereever possible, bring out some essential fact in the development of the type. Ten schools have applied for the first of the collections, now ready for distribution.

A REPORT on technical high schools in Germany by Dr. Frederick Rose, British Consul in Stuttgart, has been issued by the British Foreign Office. According to an abstract in the London *Times* Dr. Rose begins by referring to his previous report on chemical instruction and the chemical industries in Germany (No. 561 in the same series), in which he demonstrated that by means of thorough chemical education in the universities and technical high schools Germany had in the course of half a century risen to the front rank in the nations of the world in chemical industry, so that her chemical products are now valued at about 50 millions sterling yearly—a sum which is considered as the interest accruing from the capital invested by the country in chemical education. The present report deals with the technical high schools of the country generally, as their part in the industrial progress of the country has been very important. At present there are nine of these institutions in Germany—at Aix, Berlin, Brunswick, Darmstadt, Dresden, Hanover, Karlsruhe, Munich and Stuttgart, while one at Danzig is to be opened shortly, and one at Breslau in the course of three or four years. Most of them date from the years succeeding the fall of Napoleon, when they were founded as small trade or technical schools; then they passed into the stage of polytechnic schools, and during the last quarter of a century into that of technical high schools, while they now grant degrees and rank with the older universities. They are all in towns of 100,000 inhabitants and upwards, and their growth and progress are coincident with the transformation of Germany from an agricultural to an industrial state. The German universities have always taught some branches of pure and applied science, but they have always regarded the economic application of science as inferior to research in pure science; chemistry is an exception to this rule, but the idea of technical education has never been able to assert itself as equal with the pursuit of knowledge and science, and hence the necessity for the technical schools.

WATER-SUPPLY Paper No. 80, now in press, United States Geological Survey, by Mr. George W. Rafter, deals with the subject of the relation of rainfall to run-off. Some of the many conclusions of the paper are here given. Mr. Rafter holds that there is no general expression giving accurately the relation of rainfall to run-off, every stream being, in effect, a law unto itself. The cause of rainfall, beyond the cooling of the air below the dew point, is not very well understood; and it is uncertain whether rainfall is in any degree increasing. Rainfall and run-off records are conveniently divided into storage, growing, and replenishing periods, a large percentage of the total water supply running off during the storage period. The run-off of streams has been generally overestimated. Evaporation is a persistently uniform element, and streams with large evaporation are, so far as known, always deforested. Ground water must be taken into account in order to understand all peculiarities of stream flow, and a very important effect of forests is in increasing the ground-water flow, so that it may be said that the removal of forests notably decreases minimum stream flow. It is uncertain whether forests in any way influence the quantity of rainfall. As a broad proposition merely it may be said that catchment areas from which municipal water supplies are drawn should be heavily forested. Nevertheless, Mr. Rafter thinks that it would not be a good investment for the city of New York to undertake to reforest the Croton catchment area; and for this opinion he assigns the following reasons: To acquire the entire watershed—a necessary prerequisite—and to plant it in trees would cost, on a very conservative basis of estimate, about $24,000,000. There would be some consequent increase of water supply after about 30 years, but 120 years would be needed to realize the full effect of forestation and to produce the estimated resulting additional supply of about 75,000,000 gallons per day. By the expiration of the 120 years, however, the original cost compounded at three per cent. interest would

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amount to about $780,000,000, a sum out of all proportion to the resulting daily increase of water supply. Hence the attempt to increase the water supply by forestation of the Croton catchment area is inexpedient.

UNIVERSITY AND EDUCATIONAL NEWS.

ANNOUNCEMENT of a gift of $150,000 from Mr. J. Ogden Armour was made at the convocation exercises of the Armour Institute of Technology on June 19.

THE committee appointed by the Columbia University council to prepare a report on 'what celebration, if any, should be held on the one hundred and fiftieth anniversary of the foundation of the corporation,' which occurs on October 31 next, has made the tentative suggestion that the commemoration last from October 25 to November 1. On October 25, 26, 27, 28, a series of colloquies, conferences and lectures is proposed to be delivered by eminent European and American scholars. On October 31 there will be a luncheon and reception in honor of the guests, and an address, historical in character, by the president of the university.

ARRANGEMENTS have been made between the Western Reserve University and the Case School of Applied Science permitting students to complete their academic and engineering courses in five years.

COLGATE UNIVERSITY has given up its degree of Ph.B. and will hereafter give the B.A. degree without required Greek.

CHARTERS have been approved incorporating independent universities at Manchester and Liverpool to be known as the Victoria University of Manchester and the University of Liverpool.

ON June 18 the corporation of Brown University voted to establish a graduate department and elected Professor Carl Barus as dean.

DR. LE BARON RUSSELL BRIGGS, professor of English and dean of the Faculty of Arts and Sciences at Harvard University, has been elected president of Radcliffe College to fill the vacancy caused by the resignation of Mrs. Agassiz.

PROFESSOR M. E. COOLEY, of the engineering department of the University of Michigan, has been offered the deanship of the Engineering School of the University of Wisconsin.

DR. FREDERICK E. BOLTON, professor of education in the University of Iowa, was offered the presidency of a normal school at Manila, Philippine Islands, but has declined the position.

THE Rev. Dr. Smith, for the past twenty years president of Trinity College, has resigned. Dr. F. S. Luther, Jr., professor of mathematics and dean of the faculty, is acting president.

DR. J. J. R. MCLEOD, assistant demonstrator of physiology at the London Hospital, has been appointed professor of physiology at Western Reserve University, occupying the chair made vacant by the removal of Professor G. N. Stewart to Chicago.

PROFESSOR C. H. ROBINSON has resigned the chair of physics at Rochester University.

H. C. IVES, instructor in Worcester Polytechnic Institute, has accepted an assistant professorship of civil engineering in the University of Pennsylvania.

THE three scholarships available for members of the Harvard summer course in geology in the Rocky Mountains open to general application have been assigned to Chas. W. Brown, of Rhode Island, graduate of Brown University and instructor; W. S. Tower, of Massachusetts, student in Harvard University, and P. H. Cormick, of Texas, student in the University of Tennessee.

MR. ALFRED HUGHES has been appointed professor of education at Birmingham.

MR. CARVETH READ has been appointed to the Grote professorship of philosophy of mind and logic at University College, London, in succession to Professor James Sully.

M. DANIEL has been elected to a newly-established chair of agricultural botany at the University of Rennes.

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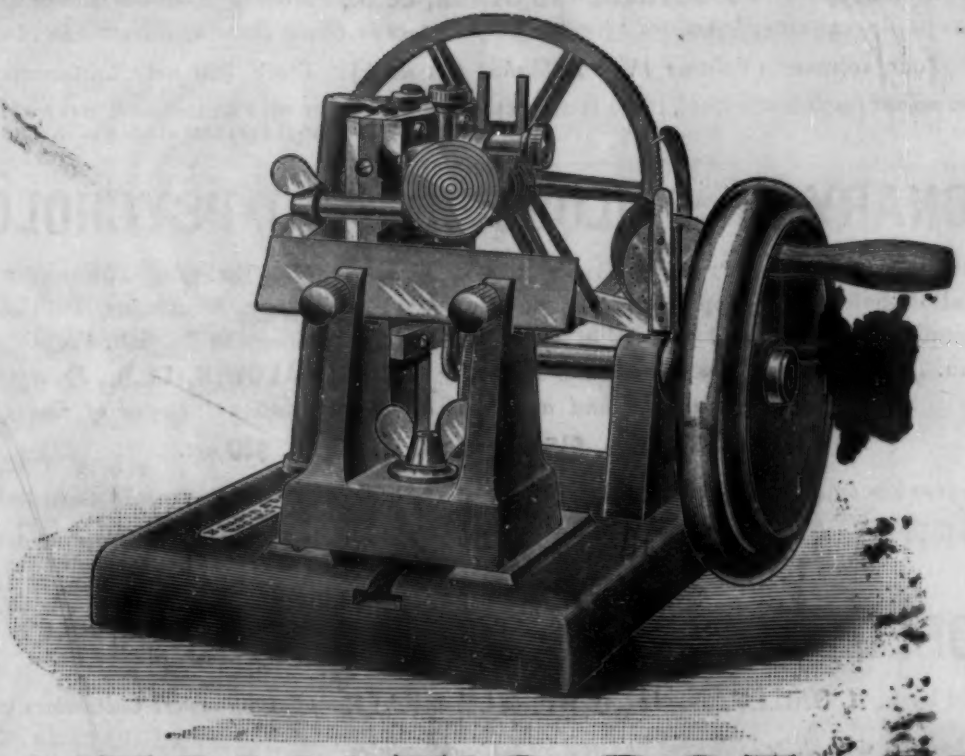

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